Letters to the Editor.

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The Internal Physics of Metals.

I NOTICE in an article in NATURE of April 14 on "The Internal Physics of Metals" considerable importance is given to the idea of the existence of an amorphous or vitreous layer between the crystals composing metals and alloys, and certain seasonal changes in them are attributed to the presence of this layer. The remark is made that until 1919 the phenomenon of "season cracking" was considered to be an isolated one, and recognised only in brass.

"Season cracking" is, however, only an extreme case of the secular relief of strain which occurs in all metals which have been subjected to cold working.

It may be of interest to some of your readers to learn that this state of strain in cold-worked metals and its cause had been dealt with in a paper read before the Faraday Society in 1904, while in the May lecture to the Institute of Metals in 1911 an illustration was exhibited of the partial relief of strain by cracking which had occurred within twenty-four hours. In these papers, and in others communicated to the Royal Society, the change from the crystalline to the vitreous state brought about by mechanical disturbance and "flow" was shown to occur in metals and other crystalline substances. The effects of this change of state on the chemical, electrical, acoustical, optical, and mechanical properties of the substances were dealt with, and were all shown to be associated with a condition of strain which could be completely relieved by the restoration of the fully crystallised condition by raising the mass to a temperature far short of its melting point.

It was suggested that the changes of structure which are produced by the cold working of metals could be accounted for by the occurrence of liquid-like flow at all internal rubbing surfaces, followed by almost instantaneous resolidification of the liquid phase, thus producing a hard cement, binding together the broken and distorted remains of the original crystal grains. In wire-drawing, for example, an entirely new "texture" is developed even in pure metals. The crystal grains are drawn out into fibres, which are embedded in and cemented together by the portion of the metal which had passed through the liquid phase as the wire flowed through the hole in the draw-plate. Owing to the greater solubility of the metal which has flowed into the vitreous state, the first effect of a solvent on the wire is to dissolve away the cementing material and to expose the fibrous structure.

"Season cracking" seems, therefore, to depend (1) on the free flow of the metal during drawing, for the greater the production of the liquid phase the greater will be the shrinkage at the moment of resolidification, and the greater will be the resulting state of strain in the hardened metal; and (2) on the subsequent action of a solvent which, by removing or breaking up the vitreous skin and cementing material, will enable the elastically strained fibres or layers to spring apart. The solvent may be mercury, or an acid or saline liquid, or acid vapours or even water vapour in the atmosphere. A piece of hard rolled metal foil is thoroughly springy and resilient, but this resilience is completely removed and the foil becomes soft if the vitreous layer on the surfaces of the foil is removed by a solvent.

GEORGE BEILBY.

April 21.

SIR GEORGE BEILBY'S work on the generation of amorphous metal as the result of "flow" during plastic straining or surface polishing of metals is so well known and appreciated that a contribution from him to the discussion of "season cracking" is very welcome. At the general discussion on this subject the fullest reference to his work in first indicating the existence of metal in an amorphous condition was made. Sir George Beilby's letter, however, appears to be based mainly upon the brief article in NATURE of April 14 rather than on the full discussion of the subject, of which that article could not give more than a very brief account from one particular point of view. No doubt for that reason Sir George Beilby has apparently missed some of the main points of the discussion, and has made a suggestion with regard to "season cracking" which is not easily reconciled with the known facts.

Thus, one reason why special importance is attached to an amorphous inter-crystalline layer or "cement" which exists in entirely unstrained metals is that fracture in "season cracking" follows the boundaries of the original crystals, and does not follow the lines of flow or slip within the crystals upon which Sir George Beilby's amorphous metal is formed. Further, it has now been clearly shown that fracture essentially of the nature of "season cracking" can and does occur under the prolonged application of external stress in fully annealed, or even cast, metal in which there has been no formation of Sir George Beilby's amorphous metal as the result of plastic strain. It follows, therefore, that the amorphous metal generated by plastic strain must be regarded as playing only an indirect part in the phenomena of "season cracking," that part being so to stiffen and harden the metal that it can carry an internal stress high enough to bring about the gradual separation of the crystals along their original boundaries.

With regard to the statement that prior to 1919 "season cracking" had been regarded as an isolated phenomenon confined to brass, this is true in the sense that until the publication of Rosenhain and Archbutt's paper it had not been recognised that this type of inter-crystalline fracture under prolonged stress could occur in other metals than brass, and possibly nickel-silver, whereas it was then shown that it also occurs in lead, in aluminium alloys, and even in steel.

THE WRITER OF THE ARTICLE.

Biological Terminology.

MR. CUNNINGHAM writes (NATURE, February 24, p. 828): "It is a mere matter of terms and synonyms. The modern biologist would say that the normal hand was hereditary, or innate, or due to certain factors or genes in the chromosomes which usually are handed on unchanged 'down the germ-tract'; that the sixth digit was a mutation, due to some change in the genes in the chromosomes, and therefore gametogenic; and that the scar was due to an injury which resulted in regenerative processes producing new tissue. . . . Sir Bryan Donkin writes that like exactly begets like when parent and child develop under like conditions; if we say, then, that the differences due to unlike conditions are acquired characters, what is the objection?"

The objection is that what is true of individuals is not necessarily true of characters, and that Mr. Cunningham's thoughts drift to and fro, now comparing individuals and now characters. Moreover, he makes distinctions where there are no differences. As a consequence, he is convinced that I contradict myself, and so on. Taken by itself, not a statement he makes is incorrect. Taken as a part of a whole, every statement is incorrect. It is quite true that a hand and a sixth digit are germinal, but the scar also takes origin in germinal potentiality. It is true that the scar is a response to the stimulus of injury, and in that sense acquired; but injury is not the only form of nurture, and hands and sixth digits are as much products of nurture and as much situated in the soma as scars.

Is not the following true?—(1) All likenesses between individuals are innate *and* acquired. For example, men have similar hands because, (*a*) having started with similar germinal potentialities, and (*b*) experienced similar nurtures, they have (*c*) developed similar characters. (2) All unlikenesses between individuals are innate *or* acquired. Thus a sixth digit indicates an unlikeness (variation) which has a germinal origin; for under similar nurtures the individuals develop differently. A scar indicates an acquired or somatic unlikeness (modification); for this unlikeness develops only when unlike nurture is experienced by the individual. (3) All characters as such (*e.g.* when compared together) are innate *and* acquired. Thus a hand is founded on germinal potentiality, and, therefore, is innate; it develops under the influence of nurture, and, therefore, is acquired; and it is situated in the soma, and, therefore, is somatic. The same is true of every character that can be thought of. It follows that while it is correct to distinguish differences between individuals by the terms "innate" and "acquired," it is incorrect so to distinguish characters. A sixth digit indicates an innate difference, but is not in itself especially innate. A scar indicates an acquired difference, but is not in itself especially acquired. If the matter be considered, it will be found that while some biology (*e.g.* the theory of natural selection and the Mendelian theory) is founded on the belief that differences between individuals are innate *or* acquired, much the greater part of biology—or, at least, of biological literature (*e.g.* the Lamarckian and Neo-Darwinian hypotheses)—is based on the assumption that all characters are so distinguishable.

It is admitted that in the germ-cell are, not the characters of the individual, but only potentialities for developing them in response to fitting nurture. Therefore, nothing but potentialities can be transmitted. It follows that when, using a colloquialism which is pardonable, since it neither deceives nor confuses, we say that a child "inherits" his parent's hand, we can mean only that the child, having inherited a like potentiality, has under similar conditions developed a similar character. We then mean that the child is like the parent both by nature and by nurture, both by inheritance and by acquirement. If we used our words with the same meanings, we should say that a child inherits his parent's scar when he develops it under the same conditions as the parent did (in response to injury). The child would then be like the parent both by nature and by nurture. He would really have "inherited" in the only sense in which the word has meaning. But, misled by his misuse of words, the biologist will have none of this. He would regard the scar as inherited only if the child reproduced it in a way in which the parent did not and could not have produced it, only if the child were unlike the parent both by nature and by nurture, only if the child had varied so profoundly and improbably from his progenitors that the scar, this ancient and vitally useful product of evolution, is now produced (and the whole course of evolution upset) under some other influence as a useless and burdensome thing. The misuse of the words "innate," "acquired," and "inherited" conceals the enormity of the notion and gives it an air of probability. As a consequence, biologists have debated for a century as to whether evolution follows the "transmission" of "acquired" characters, and to-day biologists using "exact methods" are trying to ascertain what characters are "innate," and therefore worthy of the attention of the student, and what "acquired," and therefore unworthy of his attention.

When employed to describe differences between individuals, the words "innate," "acquired," and "inherit" are used intelligibly with their ordinary dictionary meanings. When applied to characters they cannot have these meanings. They have then no meanings, or technical meanings. It is claimed that they have the latter. But, as has appeared in this correspondence, no technical meanings can be thought of which accord with past or present usage. Moreover, the claim is unhistorical; for, as may be seen by an examination of literature, biologists have never intended to give their words technical meanings. Their very synonyms, "germinal," "blastogenic," "somatogenic," and the like, were coined to give greater definiteness to the naive belief that, while "some characters have their representatives in the germ-plasm," others are products of "heat, light, moisture, and the like." Historically, all biologists have limited the term "acquired" to characters which develop in response to glaringly obvious stimuli, and applied the term "innate" to all other characters. For example, the musculature of the blacksmith has been termed "acquired," while those of the child, the youth, and the ordinary man which have developed in response to precisely the same stimulus (use) have been termed "innate."

"Innate," "acquired," and "inherit" are the chief terms of biology. We see that the first two have sometimes clear meanings and sometimes no meanings, and that "inherit" sometimes means "inherit" and sometimes its direct opposite, "vary." I daresay that most readers of this correspondence think I am engaged in a mere logomachy. But with the chief terms in such a state of vagueness and confusion, how is it possible to build a science? Confusion is sure to follow. It has followed. As Dr. Norman R. Campbell has well said (Nature, April 21, p. 234): "Accuracy of thought is intimately dependent upon the constancy of the meaning of the words used to express it." Consider the chaos of biological sects and opinions. Consider the controversies, always unending in the face of abundant evidence, and, therefore, as clearly products of mere prejudice as religious or political disputes. Consider the fact that, alone among interpretative sciences, biology has no body of truth accepted by all its students with the sole exception of the supposition that living beings have arisen through evolution. Consider the parochial littleness of biology, which has more tremendous problems ripe and ready for solution than any other science. Consider the enormous masses of neglected evidence—for example, that available from physiology and pathology and that which demonstrates the evolution of the power of developing in response to functional activity. Consider what happens when a humble outsider such as myself brings his difficulties to biologists. He is told pontifically that he is doing harm, or conceitedly that biologists are quite capable of conducting their deliberations without his help, and so on. The feelings of awe and admiration excited in the humble inquirer are then likely to be—well, of no importance.

What is biology? Who are biologists? So far as I am able to judge, biology is commonly regarded as a side-show of natural history; and any zoologist and botanist is supposed to be, *ex officio*, a biologist. But biology is an interpretative science, and systematic zoology and botany are purely descriptive. They may furnish valuable evidence, but they do not necessarily do so. The zoologist or botanist trained in observation and description may interpret skilfully, but such skill is not a necessary outcome of his studies. Zoologists and botanists have themselves proclaimed the inadequacy of their evidence by founding the experimental and biometric schools, which began as violently opposed sects, and so continue.

To my thinking, biology is that science which sits at the hub of all the studies concerned with life—zoology, botany, physiology, psychology, medicine, bacteriology, embryology, anatomy, palæontology, sociology, even pedagogy and history—gathers evidence from them all, and deals especially with problems too big or deep for these individual studies, *e.g.* problems of heredity, evolution, development, and the like. If the biologist be controlled by the rules which ordinarily guide scientific procedure—for example, the rule that all verifiable and relevant facts (no matter how, or by whom, or when, or where collected) are equal before science, by the rule that all hypotheses must be crucially tested (*i.e.* so tested by *fresh* and *unlike* facts that every alternative hypothesis is rendered inconceivably as true), and by the rule that a fully established theory must be accepted as true regardless of all preconceptions—then a very splendid future immediately awaits not only biology, but also science in general; for the claim of science to the deciding voice in the settlement of numerous burning problems of immense importance will become irresistible.

By way of demonstrating that I am not vapouring, I shall venture to give one or two examples of evidence ignored and problems neglected by biology "as she is spoke" in a future communication. Meanwhile, there is a little more in Mr. Cunningham's letter with which it is necessary to deal. He says that naturalists would not admit that man, as an animal, is "higher" than an insect. It is pleasant to find him so careful of meanings; but will he please excuse the expression as "technical"? It is in common use and deceives no one. He declares that I give no evidence of the evolution of the power of developing in response to use. Is there any need? A man develops from birth to death mainly in response to this influence; does Mr. Cunningham believe that a butterfly develops in the same way to an equal extent? Consider mind. All learning, thought, intelligence, and reason depend on the growth of the mind through functional activity. Mr. Cunningham has done magnificent work on hormones. Does he think a beetle could learn what he has taught? What is intelligence but a power of developing in response to experience, of growing mentally in response to functional activity? What is stupidity but a "natural" or "acquired" incapacity so to profit? A human infant can learn, but has not learned. A human idiot cannot learn, and has not learned. A normal man can learn, and has learned. Almost all that separates the normal adult mentally from the infant and the idiot develops in response to use. The perfect idiot cannot even learn to walk or to speak. From the human point of view every dog is an imbecile, every cat an idiot, every beetle a perfect idiot. The beetle is more efficient than the human idiot merely because he is more completely equipped with instincts and instinctive actions, which, unlike human habits, habitual actions, and the rest, do not develop through use. For example, the beetle does not learn to use his limbs. Does not the difference between man and the beetle indicate an evolution of the power of developing in response to use? What more evidence does Mr. Cunningham want?

G. Archdall Reid.

9 Victoria Road South, Southsea, April 23.

The "Flight" of Flying-fish.

In Nature of April 21 Prof. Wood-Jones presents some interesting observations on the "flight" of flying-fish made from an especially favourable vantage-point—the overhanging bow-sheaves of a cable ship.

While crossing the Gulf of Mexico on various occasions I made some observations on the same subject with the aid of powerful binoculars (Goerz prismatic, magnifying 12 diameters). With these I had been used to following birds in flight, and with a little practice found that I could keep flying-fish under continuous observation during their passage through the air.

I can confirm Prof. Wood-Jones's account in the following important particulars:—

(1) The initial impulse is always given by rapid lateral strokes of the tail as the fish leaves the water. Since the lower lobe of the caudal fin is elongated, the fish can continue to propel itself in this manner for some time while the whole of its body is out of the water. On very calm days the moving lower lobe of the tail leaves a track in the water in the form of an interrupted line. Presumably the interruptions represent the times of violent lateral motion. The uninterrupted sections of line are each 2–3 in. long, the interruptions rather longer, the whole line often continuing for 5–6 ft. After this, of course, the fish rises wholly into the air.

(2) The fish may regain impetus by again vibrating its tail when it has dropped far enough for the lower tail-lobe to be once more in the water. Fresh impetus may be gained in this way once, twice, or even three times in a flight without the body ever touching the water.

(3) The pectoral fins are usually held stiffly out, as Prof. Wood-Jones states, and act as planes. I have, however, on several occasions seen rapid vibration of the pectoral fins for a short period; but whether this was actual "flight," as I at the time supposed, or whether it was due, as Prof. Wood-Jones suggests, to a passive vibration caused by the air meeting the fin at a certain angle, I am unable to say. The impression made upon me at the time was that the *normal* means of propulsion in air was the tail, but that the pectoral fins *could* be used as supplementary flying organs on occasion. Of the truth of the first part of this impression I have no doubt whatever; Prof. Wood-Jones's anatomical studies make me doubt the second part. However, a good binocular in the hands of anyone trained to field observation will put the matter to the test.

Julian S. Huxley.

New College, Oxford, April 25.

The Concept of "Space" in Physics.

Prof. Eddington (Nature, April 14, p. 201) expresses well the properties that a substratum of matter, light, and electric force should have, and the reasons for combining space and æther, the two different, but always co-existing, substrata of the older physics, into one. What is not clear is why he stops there. The ancient rule, "Entities are not to be multiplied beyond necessity," is as applicable now as ever. If a physical æther is to be postulated, it is for those who advocate it to show their reason for doing so,